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DIASTROPHISM AND THE FORMATIVE PROCESSES

XIII. THE BEARINGS OF THE SIZE AND RATE OF INFALL OF PLANETESIMALS ON THE MOLTEN OR SOLID STATE OF THE EARTH

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In the last article of this series[1] it was found (1) that the solar gases, as they were expelled to form the planetary systems, were so mixed that they were unfitted to form solid bodies such as the terrestrial planets, the planetoids, and the satellites, until after they had been sifted by a selective process, (2) that the sifting process introduced such a serious departure from familiar modes of gaseous condensation as to require reinterpretation, (3) that the process of concentration was also complicated by inherited motions, (4) that it was still further conditioned by the formation of precipitates and precipitate aggregates, and (5) that the planetary cores, while in process of formation, were subjected to viselike squeezing, more intense below than above, followed by partial relaxation, so that selective extrusion attended the closing processes, involving the ascent of the lighter mobile matter and the compression and reorganization of the rest, thus contributing

[1] "Diastrophism and the Formative Processes. XII. The Physical States of the Planetary Nuclei during Their Formative Stages," *Jour. Geol.*, Vol. XXVIII (1920), pp. 473–504.

toward high density, rigidity, and elasticity in the central parts. It was further found that the shapes of the planetary cores were influenced from the very outset by the gyratory system of circulation that attended their formation, and that they thus failed to take on strictly spherical forms, so that they were likely to yield unsymmetrically to the heavy masses later built upon them by planetesimal growth. Even the primitive circulation thus had its influence on the diastrophism that developed much later.

Let us now consider the planetesimal growth. This involves (1) a study of the nature of the planetesimals at the start, (2) the conditions that affected the mode and extent of their growth, and (3) the modes and rates of their infall and the effects of these on the molten or solid state of the earth, as also on its content of explosive gases.

THE NATURE OF THE PLANETESIMALS AT THE START

The way in which the planetesimals are supposed to have arisen has been made clear in previous articles, but it will facilitate our present study to note that they took their starts from two main sources: (1) solar molecules driven into orbits by the original solar expulsion, and (2) molecules thrown out into orbits from the nuclei later by molecular interaction. There were other sources of planetesimals, but they may be neglected here. In both classes the planetesimals started as molecules chiefly. To some extent they may have been newly formed precipitates from the solar gases, or precipitate aggregates formed by the union of the fresh precipitates. Such precipitates are thought to form in the sun's photosphere now. They would be likely to have been formed by the expansion of the solar gases just after these emerged from solar pressure. The essential point here is that, whether molecules, precipitates, or precipitate aggregates, they were minute. Whether they afterward grew to notable sizes depended on the conditions that controlled their later history. Chief among the controlling influences were the dynamic properties given the planetesimals by their expulsion, and the gravitative stresses that controlled the field into which they were driven. It is to be kept ever in mind that they were bodies projected into swift independent flight, each

in its own path under control of its own inertia and the gravitative stresses of its environment. The planetesimals shot out from the nuclei had the simpler history, and are easiest followed to gain typical pictures of planetesimal behavior as a basis for estimates of their modes and rates of infall.

Let us picture the earth nucleus as pursuing a nearly circular orbit about the sun while certain of its outer molecules were escaping from it in various directions by reason of exceptional velocities given them by cumulative successions of rebounds from favorable collisions. It is easy to fall into the error of supposing that these molecules, thus escaping in different directions, would take orbits quite discordant with that of the nucleus and thus pass into the meteoritic rather than the planetesimal class. As constituents of the nucleus, they already had motions relative to the sun, and of course carried these with them they went into orbits of their own, except in so far as these motions were reduced or increased by their ejection from the nucleus. The velocity of the nucleus in its orbit should have been of the order of eighteen miles per second, and that of all the molecules of the nucleus about the same, some a little more, some a little less, by reason of their participation in rotation, et cet. It was the new and additional velocity which the escaping molecule had been given, *measured at the border of the sphere of control of the nucleus*; which determined its orbit after it had escaped. Only in extremely rare cases would molecular interaction give to an escaping molecule a speed greater than the parabolic velocity respecting the nucleus, that is a velocity sufficient to carry the molecule to infinity so far as the restraining attraction of the nucleus was concerned. The parabolic velocity of even the full-grown earth at the border of its sphere of control is 1.75 miles per second, so that we leave a large margin of safety if we assume that molecules almost never were shot away from the border of the sphere of control of the nucleus at more than two miles per second. Now, as the nucleus was moving at eighteen miles per second relative to the sun, a molecule shot directly backward would still have a velocity of sixteen miles per second relative to the sun, and a molecule shot directly forward would have a velocity of twenty miles per second relative to the sun, while

those shot out at lesser velocities, or sidewise at various angles, would have intermediate velocities. All would therefore be moving in the same general direction as the nucleus and their orbits would still be similar. The molecules in these new orbits would therefore be planetesimals, because they would revolve about the sun in orbits similar to those of the planets. The kinetic theory of gases requires us to suppose that molecules of the lighter gases escaped from the outer border of the earth-nucleus with some degree of frequency while it was hot and diffuse, and that such molecules have continued to escape from the outer border of the earth's atmosphere ever since, but much less frequently. And so of all other planets that have atmospheres, and of the sun as well. Practically all the molecules that thus escaped into orbits still remained within the sphere of control of the sun and were liable in time to be picked up again, so that this whole system of escape and recovery constitutes a mode of exchange of atmospheric material between the domains of the sun and the planets. It contributes to the maintenance and equilibrium of our atmosphere as elsewhere set forth.[1]

Let us now look to the gathering in of the planetesimals in this typical case, for that is the vital point here. Under the laws of mechanics the planetesimals shot forth in this way would come back to the virtual points of their escape at the end of each revolution in their new orbits, unless they were diverted by some intervening influence. From this it is easy to jump to the conclusion that they would all soon be picked up again by the nucleus, but not so, in general. They were nearly all thrown into larger or smaller orbits and that determined the *time* at which they should get back to the point of their origin. In most cases this was either earlier or later than the return of the nucleus and hence recapture was avoided. Figure 1 and the accompanying periodic data, prepared by Dr. MacMillan, make this very clear. From the point A at the top of the figure let one molecule be shot forward at a speed 10 per cent greater than that of the nucleus and let another molecule be shot backward so that its velocity shall be 10 per cent less than that of the nucleus. The first molecule will take the outer

[1] *The Origin of the Earth* (1916), pp. 13–17.

orbit, the second, the inner orbit. The molecule in the inner orbit will get back to *A* in .77 of the time required by the nucleus to reach this point, while the molecule in the outer orbit will require 1.424 times that period, i.e., if it takes the nucleus three hundred and sixty-five days to complete its orbit, the planestesimal that was shot backward and took the inner orbit would reach *A* eighty-four days ahead of the nucleus, while the molecule that was shot ahead and took the larger orbit would return to *A* one hundred

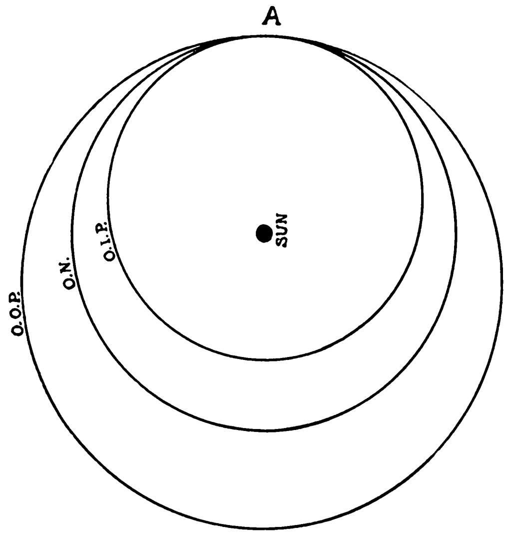

FIG. 1.—O.N. represents the orbit of the nucleus; O.O.P., the orbit of the outer planetesimal; O.I.P., the orbit of the inner planetesimal. Periodic times, earth = 1; inner planetesimal = 0.77; outer planetesimal = 1.424 (MacMillan).

and fifty-five days after the nucleus had passed. There was therefore no immediate danger of collision and recapture in either case. Only by waiting for a concurrence of the schedules, which would be liable to be thwarted by perturbations, or by a protracted series of orbital shiftings, or by changes of orbital form or dimensions, could this be brought about.

Were these planetesimal molecules likely to unite with one another in the course of their flights and so grow to larger sizes? The figure illustrates this point also. It is very obvious that the two planetesimals specified have no opportunity at all to unite

with one another until they return to the vicinity of A. But one of them would reach A two hundred and thirty-nine days earlier than the other. Even in this specially favorable case when they had a common node there was no immediate opportunity for union. Of course cases of less divergence could be chosen in which there was a nearer coincidence of orbits and of time schedules, and if there were many orbits there would be some real crossings farther from the nucleus, but those chosen illustrate the prevalent fact that even though such bodies have similar orbits and sometimes actual crossings, they may yet remain independent for long periods. Their mutual attractions would, in general, aid in bringing this about ultimately, but instead of this they might be brought into co-ordinate orbits like those of the earth and moon and revolve together in harmony indefinitely. At best the process was likely to be a very slow one. The picture of molecules drawn directly together, as in the case of static bodies or of gases, is very commonly substituted for the real case, and is very misleading. When all possible cases are considered, as well as the multitude of planetesimals, there are enough chances of collision and coalescence, especially with the nuclei, to make the process of ingathering effective in the course of long periods, but in its very nature it cannot be a speedy process. When planetesimal molecules or even precipitate aggregates collide, rebound would be more likely to follow than coalescence, unless they were electrically charged. Coalescence almost inevitably follows collision with nuclei but not encounters between planetesimals.

This simple example of the evolution and behavior of planetesimals illustrates the mechanism by which they are maintained and the contingencies of their capture or their mutual coalescence, where the conditions are exceptionally favorable. For the case most important in the formation of the earth, we must turn to the solar molecules which were driven directly into orbits by the original propulsion from the sun under the stimulus and attraction of the co-operating body. These were subject to the law of return to the points of their origin, but they were greatly diverted by the pull of the co-operating body and so largely lost all such systematic relations to a given nucleus as those that made the previous case

so simple and instructive. The orbits in this case were distributed through greater space and more irregularly, and hence their coalescence with one another and their capture by the planetary nuclei, as a rule, required greater changes in the forms, dimensions, and attitudes of their orbits. We will turn to concrete specifications and numerical values presently.

THE SIZE OF PLANETESIMALS NOT IMPORTANT IN RESPECT TO MELTING EFFECTS

Lest we stress the growth of planetesimals too much, it is prudent to observe at once that the sizes of the planetesimals were not matters of vital moment so far as the total energy-effect of their infall was concerned, for whatever was gained by concentration of mass was lost by less frequent infalls. On the whole, less energy available for conversion into heat was carried into the earth by the united planetesimals than by the same mass ununited, for in coming together, energy of motion was converted into heat and this was dissipated at the point of union in open space; the combined mass carried so much less energy into the earth-core. However, whenever combination took place, there was relatively less resistance and heating of the atmosphere in plunging through it, and so relatively more heating of the surface of the earth. As we shall see a little later, however, the chief effect at the earth's surface was lateral dispersion and an elastic or explosive reaction, resulting in a great scattering of débris with little obvious melting. None the less, we shall consider the melting effects of large planetesimals as well as small ones.

THE LIGHT SHED ON SIZE BY EXISTING PLANETESIMALS

It has been shown in previous papers[1] that the union of molecules and the growth of small aggregates could take place with more or less facility up to a certain order of size, but that beyond such order conditions unfavorable to further growth arose and increased relatively, so that indefinite growth was probably limited, as a general rule. It appeared that chemical, electrical, and

[1] Article X, this *Journal*, Vol. XXVIII, No. 2 (February–March, 1920), pp. 140–44.

cohesive attractions functioned effectively in the early stages; but that fragmentation, abrasion, and exfoliation came into increasing effectiveness as larger sizes were attained. Theoretically, then, growth from the minute state at which the planetesimals started, took place presumably up to limited sizes with relative facility, beyond which the presumption of much larger growth was adverse, except under unusual conditions. Theory, however, does not define at all closely where the balance between the opposing agencies was to be found, and so we turn to naturalistic evidence which is more decisive.

1. *The zodiacal planetesimals.*—It is an accepted view that the zodiacal light is due to the reflection of solar light from minute solid or liquid particles distributed in a lenslike form about the sun. The central plane of the lens is essentially coincident with the common plane of the planetary system. The outer border of the lens reaches to some undetermined distance beyond the earth. Under favorable conditions, it is possible to trace the counterglow (*Gegenschein*), on the side of the earth opposite the sun, into continuity with the zodiacal light on the sunward side. It is not improbable that the edge of the lens is extremely attenuated and extends indefinitely outward in the plane of the planets. The form and extent of the lens in the planetary plane make it scarcely less than certain that the particles are sustained by orbital dynamics, and that the orbits are of the planetary type and that hence the particles are planetesimals. This warrants us in turning to them for light on the sizes and masses of planetesimals. Their testimony is obvious for they are certainly quite small.

Though they envelop the earth and must in many cases be quite near, the individual planetesimals are too small for detection. Although they are certainly very numerous, their joint mass is not known to affect the motions of any body. They are interpreted either as remnants of the original planetesimal system, or as more recent products due to the projection of solar matter so close to the planets that it is drawn forward by them into elliptical orbits about the sun. If the first view is correct, or in so far as it is correct, the planetesimals are exceptionally old and should

have reached the fullest growth to which they are ordinarily subject. If they are of more recent origin, they merely bear testimony to the common size to which planetesimals of the younger order attain. But as they are so obviously minute their testimony in either case is weighty.

2. *The satellitesimals of Saturn's rings.*—Satellitesimals are merely special forms of planetesimals. It is convenient to distinguish between them in certain cases, while in other cases the generic term planestesimal is most satisfactory. In the Saturnian case, they are notable for their very close association with one another and for their definite borders. These hint at a special origin, perhaps the disintegration of a satellite by the differential attraction of Saturn, since they lie within its Roche limit. At any rate, the closeness of the individual satellitesimals to one another gives them rather pointed bearing on the question of growth to large sizes, for their nearness to one another should favor this, if mutual attraction has any appreciable effect. According to Bell's studies of their albedo, they are chiefly very minute particles.[1] Only rarely is there evidence of masses reaching as much as a meter in diameter. Trituration as a consequence of their mutual collisions is probably the dominant size-controlling agency in this case.

3. *Precipitate aggregates formed from condensing gases. The chondrules.*—When the gases or vapors of stony and metallic substances mixed with lighter gases were expelled from the sun into the vacuum of interplanetary space, they must probably have been greatly expanded and cooled and the stony and metallic substances thrown down as precipitates at successive stages according as the appropriate temperatures were reached. As each gaseous substance was diffused through the others, the precipitates could at first have been little more than molecular in size, but by subsequent interaction, in the fashion of Brownian particles, the first precipitates were brought into contact with one another and in their fresh, hot, viscous states should have united into larger aggregates rather freely. In so far as they solidified, they naturally

[1] Louis Bell, "The Physical Interpretation of Albedo, II, Saturn's Rings," *Astrophys. Jour.*, Vol. L (July, 1919), pp. 1–22.

took the form of concretions or of crystals. I have ventured to suggest that this may be the mode in which chondrules—little organized bodies that enter into the formation of 90 per cent of known meteorites—were formed. Later I shall suggest that the formation of the common small meteors of the sky may have taken place in practically the same way as planetesimals, i.e., by the progressive aggregation of precipitates from the stony and metallic ingredients of solar gases shot into interplanetary space and there cooled, the distinction between the two being their orbital characters and planetary relations. If these suggestions are in the line of truth, the chondrules give very specific evidence on the usual sizes of planetesimals, for they range from the size of a walnut down to fine dustlike particles.

4. *The negative evidence.*—Concurrent with these concrete sources of evidence, supported by theory, is the significant fact that no bodies of a distinctly larger or *planetoidal* order of magnitude are seen to revolve in the region of the earth's orbit or within it. We have found reasons for suspecting that the normal dynamic stresses in this inner solar region have always been too great to permit the collective aggregation of precipitate clouds of so little mass. At any rate, the negative testimony of observation stands against any view that postulates an abundance of bodies of planetoidal size in the region of the earth or their effective participation in the formative processes of the earth or the moon.

Let us then assume that the chondrules are our best naturalistic guide in respect to the normal sizes of planetesimals. For definiteness in the computations that follow, let us assume as a convenient representative weight, one-fiftieth of a pound, say one-third of an ounce, or about 9 grams. In testing the probability, or otherwise, that planetesimal infalls would produce a molten state of the growing earth-core, it will make no essential difference whether the planetesimals were somewhat larger or somewhat smaller than this size which is adopted merely for definiteness and convenience. After inspection of the melting effects in a case thus made as nearly normal as we conveniently can, we will try to test the effects of supposedly larger planetesimals.

THE TIME OVER WHICH THE INGATHERING OF PLANETESIMALS WAS SPREAD

It is obvious that the time over which the infall of planetesimals was spread is an essential factor in determining whether the heat of their infall would melt the earth surface or not. And so if there are any naturalistic evidences bearing on this point, they should be brought under consideration at once so that they may serve as guides or tests where assumptions have to be made in trying to deduce the period of infall from the mechanics of the case. Successful study of earth history has been found to rest much more largely on naturalistic considerations than on deduction, especially when the premises involve so much that is assumptive. An approach along naturalistic lines may be found in biologic evolution combined with geologic chronology.

1. *The intimations of biologic evolution.*—It seems to be the consensus of opinion among those best fitted to judge that the portion of life-evolution that has taken place since the faunas and floras of the early Paleozoic offered a fair criterion for judgment, is of the order of one-tenth of the total life-evolution, or some such proportion. This proportion will therefore be made the basis of the time-scale used in the following discussion. It will be easy to modify the results of the computations to suit any other proportion that may be thought nearer the reality. I do not think that any other proportion which is tenable will change the general tenor of the conclusions, so far as these bear on the melting effects of planetesimal infall.

Two geologic time-scales are now in use, an older one built on estimates of the present rates of geological progress, and a newer one built on radioactive processes. For myself I regard the latter as much the more trustworthy. The former seems to me to need radical correction (1) for the exceptional speed of present denudation due to the stripping of very large portions of the surface of its native protection, and (2) for the exceptional speed induced by the present high relief of the surface brought about by recent diastrophism.[1] But let us use both scales. Those who prefer

[1] See Article VIII of this series, "The Quantitative Element in Circumcontinental Growth," this *Journal*, Vol. XXII (1914), pp. 516–26.

the old scale will doubtless concede that the *proportions* of the radioactive scale may be used safely as a means of extending the older scale over the Proterozoic and Archean eras, where its own criteria are not available. Using the radioactive scale, the beginning of the Paleozoic may be placed, in round figures, at 4×10^8 years ago, the beginning of the Proterozoic at 12×10^8 years, and the oldest portion of the Archean that has been determined in respect to age, at 16×10^8 years. Using the old scale, the beginning of the Paleozoic may be placed at 10^8 years and—using the radioactive scale for proportionate extension—the beginning of the Proterozoic at 3×10^8 years, and the earliest determined Archean at 4×10^8 years. To fill out the *total* period of life-evolution on the radioactive scale, an allowance of 24×10^8 years *previous* to the earliest determined Archean must be made, making the total life-period 4×10^9 years. To similarly fill out the total period of life-evolution on the old scale, 6×10^8 years is to be allowed previous to the earliest determined Archean, and 10×10^8 years for the whole life-period.

In thus using the proportions of biologic evolution as an indication of the period over which the growing stage of the earth was spread, it is to be noticed that, to avoid making the assigned period of planetesimal infall unfairly long by including too much of the tailing-out stage, I shall consider all planetesimals that fell during the last 400,000,000 years (radioactive scale) of the Archean era, and all that have fallen since, 16×10^8 years in all, as though they had fallen within the computed period. On the other hand, all that fell during the nuclear stage, i.e., before a definite earth-core was formed, are necessarily excluded from the estimated period of life-evolution, since the conditions were incompatible with life. No doubt the infall during the distinctly nebulous portion of the nuclear stage may have been more rapid than during the biologic stage but that does not concern us in considering the period of biologic activity preceding the earliest age-determined Archean. It is the special merit of the planetesimal hypothesis that it takes due account of biological requirements, as generously interpreted as the leaders in biological inquiry demand. The biological

evidences are regarded as among the most cogent that bear upon the duration of the early history of the earth.

Qualified and defined as thus specified, the period of effective planetesimal infall subsequent to the nuclear stage is made to range from 600,000,000 years on the old geological scale, to 2,400,000,000 years on what seems to me to be the more probable radioactive scale.

These assignments of time may impress some readers as very long, but the question is to be asked anew, are they longer than the biological evidence requires? We shall soon inquire whether they are any longer than the mechanics of the case warrant. But before passing on, it is to be noted that the interpretation of biological evolution should no longer suffer from duress due to supposed limitations of time, such as were vigorously urged during the last half of the last century by advocates of the contractional theory of the sun's heat and of other physical tenets which were really less well grounded than the biological and geological interpretations. This alien stress is now not only lifted, but a new theoretical urgency of precisely opposite import has taken its place, a seemingly imperative need to find a source of heat for the maintenance of the stars of such potency as will enable them to serve their indicated functions in the protracted history of star clusters and our stellar galaxy. For this, a stellar longevity of the order of ten billion years, or some such great period, seems to be required. Short of trespassing on some such time allowance as that, biology and geology cannot be said to be necessarily restricted for lack of solar endurance. The seeming demand of biological and geological evidences for a total earth age of three or four billion years need not be thought extravagant or unreasonable, if either class of evidence is found to really require it.

2. *The intimations of the planetesimal mechanism.*—Let us now turn to the planetesimal mechanism to see what may be its most probable time requirements. Neglecting planetesimals of high and unusual orbital range, a fair and at the same time conservative working approximation to the extent of that portion of the planetesimal field which was tributary to the earth, may be made by

taking the width of the tract now occupied by the planetoids as its breadth, and for its depth the limits of the earth's dominant attraction in competition with that of Mars on the outside, and that of Venus on the inside. These give roundly 55×10^6 miles in breadth and 58×10^6 in depth. They define the cross-section of the planetesimal ring which curved around the sun with the path of the earth-core near its center. The actual field was much larger than this, but the planetesimals outside these limits are neglected to compensate for any lateral thinning inside. The area of the cross-section was therefore roundly 3×10^{15} square miles and its curved length 292×10^6 miles. For a working case of the medium order, let the mass of the earth-core, at the beginning of the specified period of planetesimal infall, be taken as one-third of the final earth-mass, leaving two-thirds of the earth-mass in the form of planetesimals to be gathered in. It will be seen that this proportion makes the mass of the planetesimals large and favors effective infall. It is taken merely as a fair working basis without any intention of implying an opinion as to the ratio of the nuclear to the planetesimal portions which actually obtained; that may best be reserved for further study. Taking the masses and dimensions of Mars and Venus as guides, in accordance with our comparative studies (Article X), the earth-core should have had a diameter of about 6,000 miles. Its disk would then have an area of 28×10^6 square miles, roundly. This is the fleeting target which the widely scattered planetesimals must hit, if they were to take part in the earth-building, or to change the simile, this is the area of the sweeper that must gather in the planetesimals from their vast field to build its one-third mass up to a three-thirds mass.

1. As rigorous treatment is impracticable, modes of approximation are our only recourse; and so, as a simple and purely artificial first approach, suited to give a realistic impression of the immensity of the field that must be swept, let us suppose that the planetesimals stand still while the earth-disk sweeps through it at its normal speed, changing its path in such an effective way as to clean up an entirely new swath at each revolution. Even by this impossibly speedy method, 100,000,000 years, roundly, would be required.

2. To make a first approach of a natural kind that can be treated mathematically, Dr. MacMillan has suggested that the planetesimals might be treated as though they were particles of gas which would close in upon the track of the earth-core as it revolved through the center of the tract, though the dynamics of gases are radically different from those of planetesimals, and corrections must be made accordingly. To gather in all the planetesimals under these conditions would take an indefinite period; to gather in 90 per cent would require somewhat over 260,000,000 years. Keeping in mind that this is not the real case, but merely one that can easily be treated, it is worth while to note that one-fifteenth of the earth-mass would fall in after 260,000,000 years had passed and that nine-fifteenths would be systematically distributed over this period with infall greatest at the start in due proportion, but it does not give warrant for excessive concentration in the early stages. If that is assumed, it makes the more certain a non-melting rate in the later stages and the infall during these would furnish the outer shell of the earth to a depth beyond the reach of most problems of immediate geologic interest. The vital point, however, is that in this substitute case, like the real one, the laws of mechanics require a distribution of infall over long periods.

3. The next step toward the real case is the substitution of heterogeneously revolving particles for the previous gaseous particles. A gaseous organization is a failing structure in the sense that when any inner portion of it is removed the rest collapses sufficiently to fill the space. In an orbital organization no such collapse takes place, each remaining body is sustained in its orbit by its own moving force. This makes a radical difference in the rate of ingathering by a body like the earth-core in the case in hand. Those planetesimals whose paths had actual crossings with that of the earth-core would be picked up, if not disturbed by perturbation, whenever their time schedules became coincident at the crossing, but not before, normally. Those planetesimals—by far the greater number—which had no such actual crossings at the start, would circle through their independent orbits indefinitely,

if they were not thrown out by collision, which would be rare in such vast space, or perturbed by other bodies, among which the earth-core would be the most influential in most cases. But such perturbations work very slowly, and their effects on the orbits involved are not easily visualized by any except experts in orbital dynamics. It is easy, however, to see that the case is far different from the direct collapse of gaseous particles and that it must occupy much greater time. In the lack of any rigorous determination of just how much longer the ingathering process would take, we may merely note that if it be taken as no more than two or three times longer, the total period would at least equal the biological requirements given above on the older geological scale.

But such bodies in heterogeneous orbits belong to the meteoritic type, and would not arise normally from the dynamic influences postulated by the planetesimal hypothesis, nor would their aggregation give rise to planets in concurrent revolution, for lack of the requisite moment of momentum, unless it were assigned them by some supplementary hypothesis such as revolution of the whole assemblage. As in the preceding case this assumption only serves as a step toward the real case.

4. The distinctive feature of the postulated planetesimals was that *they were moving in the same general direction as the collecting body and at the same general rate of speed.* The process of collection was therefore confined to overtakes and to convergencies of orbits. The differences between this and the preceding case may be compared to the different degrees of danger of collision between automobiles when, in one case, they are running in a common direction, on the right side of the road, under fairly well regulated speeds, and, in the other case, running wildly at random in both direction and speed. So planetesimals, circling about the sun in more or less concurrent orbits, only collide and coalesce in so far as they deviate from concurrence with the rest of the system or are perturbed in their independent orbits and drawn into coalescence by overtakes or convergencies. In so far as their orbits were concurrent, the moment of momentum of the combined mass was nearly as high as the sum of the individual moments of momenta, and so, if at any stage the orbits became adjusted to one another, they

might revolve in harmony indefinitely, as do the earth and moon. It was this concurrency of movement that made the evolution of a planetary system highly endowed with moment of momentum a possibility. Therein lies the soul of the planetesimal theory. But evolution under these conditions requires great lapses of time.

But lest this be overstressed, it is to be noted that the subparallelism of orbits and the subequality of speeds gave greater effect to the mutual attractions of the earth-core and the planetesimals, and so tended either to bring them together or else into harmoniously adjusted orbits, such as those of the earth and its satellite. Compared with the much more familiar gaseous and meteoritic types, the fundamental tendency of a planetesimal system is *not so much direct concentration as concurrent revolution*, though, in so far as the nuclei are competent, they gather in the smaller bodies. It seems clear, therefore, that the time required for collecting the planetesimals would be some multiple of that assigned in the preceding case. It is not clear just how large it would be, but if taken at three or four, the total time requirement would equal the maximum estimate of the biological requirement. In the nature of the case, it should not be less, for life-evolution could not proceed until a solid core was formed and the rate of infalling planetesimals permitted a congenial temperature. At any rate, however large may be the latitude for different numerical estimates of the total time and rate of planetesimal infall, it is altogether clear that a precipitate ingathering is incompatible with the mechanics of the planetesimal system.

THE RATES OF PLANETESIMAL INFALL

a) The infall of normal planetesimals.—We have already found reasons for thinking that the planetesimals were usually small, as their name implies, and have chosen one-fiftieth of a pound as a working figure. We have also chosen one-third of the total mass of the earth as the amount of material already in the earth-core and two-thirds as the amount still in the form of planetesimals at the beginning of the specified period of infall.

The mass of the present earth is 6×10^{21} tons. There would then be 4×10^{26} planetesimals of the specified mass to be gathered

into the core to complete the growth of the earth. The earth-core, taken at 6,000 miles in diameter, would have a surface area of 3×10^{15} square feet. As there were 4×10^{26} planetesimals in all, 13×10^{10} planetesimals must fall upon each foot of earth-core surface, on the average, to build the body up to its present mass.

Now, if we take the total period of infall, as given above on the radio-geo-biologic scale, at 2.4×10^9 years, a planetesimal one-fiftieth of a pound in weight, falling upon each square foot every 6.7 days, or a little less than once a week, would have completed the growth of the earth in the time specified. It will be agreed, I think, that this does not remotely approach a rate sufficient to melt the earth surface. If there is any doubt as to the dissipation of energy following the impact of a falling body, see later discussions.

If we take as the period of infall the biological requirements as estimated on the older geologic time-scale, 6×10^8 years, a planetesimal falling upon a square foot once in about forty hours would build the earth up to its present mass in the time estimated. This again, I think it will be agreed, is not near the melting-rate for the general surface.

If we make the time of infall equal to the highest of our range of estimates from the mechanics of the case, 3×10^9 years, an average fall of a planetesimal on each square foot once in a little over eight days would suffice, or if we take the minimum of the estimates, 18×10^8, a planetesimal once in about five days would answer, in either case far from a general melting-rate.

If we fall back upon the untenable assumption that the planetesimals distributed themselves after the manner of gaseous particles—made merely as a first step in approach—and take the computed 26×10^7 years as the total time, the average rate of infall upon each square foot would be about one planetesimal in seventeen hours. Even this does not seem to be a rate that would threaten the melting of the earth, and yet it is much more rapid than is permitted by the mechanics of the real case under the basal assumptions made.

Let us now reverse the mode of inquiry by trying to approximate a rate of infall that would cause the melting of the earth surface, and then compare results with those reached in the preceding ways.

If the mass of the earth-core equaled one-third that of the present earth, an atmosphere of sufficient depth to protect its surface from the direct impact of planetesimals of the specified mass would have surrounded it. The melting of the earth must then have hung upon the competency of the infall to so heat the upper atmosphere as to melt the earth surface some miles below. About half the heat acquired by the thin upper air would have been quite promptly radiated outward and the melting left to the other half. The effect of the air on meteorites plunging into it is suggestive in this connection. As soon as a film of meteorite-substance becomes viscous enough to yield to the high pressure of the air condensed on the meteorite's front by its high speed, the film is driven backward and dissipated along the meteor's path forming the "streak" of the "shooting star." Only a very small part is melted at any one instant, or left in any one spot. Even this minute part only reaches the first stages of the molten state and hence is very quickly cooled again to the solid state. To apply this to planetesimals, it is to be noted that the mean velocity of meteorities is probably four or five times that of normal planetesimals, and their moving energies sixteen to twenty-five times as great in proportion to mass. The working picture, then, in the case in hand, is that of a little mass, one-fiftieth of a pound, making a similar but feebler streak of quickly heated, quickly cooled matter, down the center of a column of air one square foot in cross-section. This must take place in such close succession as to melt one square foot of the earth surface at the bottom of the atmosphere in spite of outward radiation. To really complete the picture, it is necessary to add that the lower atmosphere would soon be filled with the dust of the dissipated planetesimals and the melting of the surface would have to be effected through this screen. It seems clear that to effect general melting the upper atmosphere must be heated throughout to the melting-point of average rock-substance, and kept at that temperature in spite of

convection and radiation. As radiation increases with the fourth power of the temperature, it would be very effective as the red-hot stage was approached.

As the case is beyond the reach of experiment or rigorous computation, specific estimates of rate can be little more than matters of judgment. Let us therefore resort to the serial method, which sometimes leads to a decisive conclusion even when definite quantitative values are unavailable.[1] Let each reader fix upon such rate of infall as seems to him competent to produce a molten state of the earth surface under the given conditions. Let us then see how such a rate fits into the range of rates which the mechanics of the case permits. Too great a discrepancy may be about as decisive as if the precise rates were known. The working test is the final arbiter.

If one's assumption is that a planetesimal plunged into the upper end of each square-foot air-column once every second, the column would be built up to the present surface in 4,119 years. It will be recalled that our first, but wholly arbitrary and exceptionally speedy mode of sweeping up the planetesimal field required 100,000,000 years and the most speedy natural method 260,000,000. and that both of these hypothetical cases required less time than the real case.

If one planetesimal fell upon each square foot once per minute, the total time would still be only 247,140 years. The competency of such a rate to melt the earth surface would, I think, at least be open to question.

If the rate were one planetesimal per hour, the total period would be 14,828,400 years, which is about one-seventeenth of the time of ingathering required on even the gaseous assumption. Moreover this rate would give a cooling period to every column of air more than 3,000 times as long as the glowing period, estimated from the mean duration of "shooting stars."

At one planetesimal per day per square foot, the total time would be 355,881,600 years. I think it will be agreed that this rate of infall would fall far below a liquefying rate, and yet even

[1] "The Methods of the Earth Sciences," *Pop. Sci. Mo.* (November, 1904), pp. 70 and 71 ("The Method of Multiple Series").

so fast a rate of infall as this does not seem to be warranted by the mechanics of the case.

Apparently the only line of escape from the import of such a serial trial lies in postulating that the rate of infall in the earliest stages was sufficiently more rapid than the mean rate to effect melting in such early period. A declining rate of infall is, of course, to be presumed, and has been taken into account. The rate used in the computations is the mean rate for the specified accession when assumed to be distributed over only the period which *followed* the formation of the earth-core and *preceded* the earliest time-determined Archean, 16×10^8 years ago. The accessions before that period were reckoned as part of the mass of the earth-core, and the accessions since were thrown into the specified period to avoid counting the long tailing-out period of 1,600,000,000 years (radioactive scale). The period thus made the basis of computation represents an intermediate stage of infall and was given the benefit constructively of all subsequent infall. We excluded such infall as was contemporaneous with the evolution of the nucleus from its nebulous state until a definite earth-core was formed, because it necessarily preceded life-evolution, and because it is not separable from nebulous condensation and the other nuclear conditions. In connection with the irregularities of the original outburst, there may have arisen some incalculable rates of infall. These would doubtless have made themselves felt chiefly in the nuclear stages. Our endeavor was to include in the computations only the systematic ingathering into which the action settled as a secular process. The physical state of the nucleus during its evolution from a nebulous state into an earth-core has been left an open question, reserved for further consideration. Meanwhile, a molten state during that period has been treated as one of the alternatives, and as a not improbable one. The infall of planetesimals during that stage may probably have been an important factor in determining the state which actually prevailed. But all that is held to antedate the growth of the outer part of the earth. This embraces about all that has yet been brought under study in geological and biological inquiries. To reach a satisfactory basis for these inquiries is the soul of the present issue. The state of

the core does not radically affect most geological and petrological problems.

The infall of supposedly large planetesimals.—In the foregoing tests it has been assumed that planetesimals normally grew to about the same order of size as the chondrules, and that the disruptions and abrasions they suffered after reaching this size kept them down to about the order of the little masses that form "shooting stars." Let us now consider the melting effects likely to follow if the planetesimals had grown to very much larger sizes. To keep as close to the actual as practicable, let us base our first study on the phenomena of Coon Butte, or Meteor Crater, Arizona, interpreted as the work of a gigantic meteorite, or cluster of meteorites or, if you please, the nucleus of a comet, accepting as conclusive, in the main, the disclosures of the drillings, shafts, and trenches of Barringer and Tilghman. Then, let us base our second study on the craters of the moon, on the assumption—made solely for the sake of the study and without acceptance—that they were formed by the impacts of still larger bodies.

Case I. The testimony of Coon Butte or Meteor Crater.—There is no reason to think that the celestial mass whose plunge into the earth formed Coon Butte was a planetesimal, because, among other reasons, it came from the northward, an unlikely direction for a planetesimal and because its indicated velocity was probably too high. The work done by it, however, is very instructive respecting the physical effects of such a falling mass under natural conditions.

The essential phenomena are a circular rim of upturned strata, covered thickly by outthrown débris, 130 to 160 feet above the surrounding plain, inclosing a crater nearly 4,000 feet in diameter and 440 feet deep, measured from the original surface of the horizontal sandstone and limestone from which the crater was formed to the top of the present partial filling.[1] Crushed rock, mingled

[1] The following are among the more important papers on the subject: A. E. Fotte, *Amer. Jour. of Sci.*, Vol. XLII (1891), p. 413; also *Proc. Amer. Assoc. Adv. Sci.*, Vol. XL (1892), pp. 279–83; G. K. Gilbert, *13th Ann. Rept., U.S. Geol. Surv.*, Part I (1892), p. 98; *14th Ann. Rept.*, Part I, (1893), p. 187; *Geol. Soc. of Wash.* (President's Address), March, 1896; *Science* (N.S.), Vol. III (1896), pp. 1–13; O. A. Derby, "Constituents of the Canyon Diablo Meteorite," *Amer. Jour. of Sci.*, Vol. XLIX

with meteoritic matter, lies below the floor of the crater to a depth of about 660 feet. Below this, disrupted rock seems to grade into undisturbed sandstone at points between 1,100 and 1,200 feet beneath the general plain. Rock masses and clastic material, coarse and fine, were thrown from the pit and strewn over the adjacent plain for distances of one to two miles on all sides, while meteoritic matter, distributed subconcentrically, reaches out to an extreme distance of $5\frac{1}{2}$ miles. The rim and pit, while subsymmetrical, have sufficient asymmetry to indicate an infall from a northerly direction, perhaps N. NW. to S. SE. The chief mechanical effects were the formation of the crater by the breaking up of perhaps 8×10^8 tons of rock, and the hurling out of perhaps half of it, the turning up to high angles of the previously horizontal limestone and sandstone beds of the crater-border, the crushing of large quantities of sandstone to silicious rock flour, and the development of some schistosity in connection with it. The chief thermal effects were the partial metamorphism of some of the rock flour and the development of incipient fusion in other portions of it, some of this portion becoming vesicular. The crushing and heating were obviously the direct effects of the impact, the upturning of the rim and projection of the débris as obviously the effects of the attending lateral thrust and the quasi-explosive reaction that followed.

The energy involved in the mechanical effects must be subtracted from the total energy of the impact before the heating effects can be theoretically deduced. The very large sum total of these mechanical effects shows how great would be the error of computing the energy of infall in terms of heat and using that as

(Feb., 1895), pp. 101–10; D. M. Barringer and B. C. Tilghman, "First Mention of the Discovery that the Crater Is an Impact Crater and Not a Crater Produced by a Steam Explosion" (President's Statement), *Proc. of Acad. Nat. Sci.* (Philadelphia, Dec. 5, 1905); D. M. Barringer, "Coon Mountain and Its Crater," *Proc. Acad. Nat. Sci.* (Philadelphia, Dec., 1905), pp. 861–86 (issued March 1, 1906); B. C. Tilghman, "Coon Butte, Arizona," *ibid.*, pp. 887–914; J. W. Mallet, *Amer. Jour. of Sci.*, Vol. XXI (May, 1906), pp. 347–55; J. C. Branner, *Science*, Vol. XXIV (Sept. 21, 1906), pp. 370–71; H. L. Fairchild, at Tenth Session of the International Geological Congress, in Mexico, September 14, 1906, *Compte Rendu, X Session, Congrès Géol. Inter.* (Mexico, 1906), p. 147; O. C. Farrington, "Analysis of Siderite Oxides or Iron Shale," *Amer. Jour. of Sci.*, Vol. XXII (Oct., 1906), pp. 303–9.

a measure of the melting effects. This would be a tempting line of attack but is quite inadmissible because the mechanical effects alone call for more energy than can be reasonably assigned to the meteoritic material found. The only safe recourse is the direct evidence. The heating effects implied by the direct evidence are singularly small compared with the mechanical effects. To a considerable, but not closely determined, extent, the crushed sandstone shows incipient schistosity with partial metamorphism, obviously a compressive effect, the heat of which did not rise to the grade of fusion. To a considerably smaller extent, if I interpret the descriptions correctly, the crushed sandstone shows the early stages of fusion, while some of this portion has become inflated and pumaceous, but no appreciable masses were left in the state of glass or other completely fused product. If fully melted matter was formed at all, it was probably dispersed by the explosive reaction. It seems quite clear that the portion which became vesicular did not become fully fused and fluent, for, in part at least, the bedding lines were not wholly obliterated. These portions seem, however, to have been rendered distinctly viscous and susceptible of inflation. This must probably have taken place during the resilience which followed the compression. The internal gases could scarcely have puffed the viscous rock while the intense pressure of the impact was on. If, on the other hand, they had remained viscous until the pressure from the falling back of the exploded débris was brought to bear, they would have collapsed, at least in all deeply buried portions. Apparently they had cooled in their inflated state while the pressure was off. It seems, therefore, that there was practically no liquid rock left when the explosive reaction was over. This is a matter of radical importance in its bearings on the question of producing a holo-liquid earth by such impacts. It shows that a very high proportion of the energy of impact was converted into another mechanical form, not into heat. There is no question about the greatness of the energy of impact; the mechanical work involved in the formation of the crater and of its rim, as also in the crushing and scattering of the débris, demonstrate that. And yet there is no evidence that this violent impact left even the smallest pool of lava. *The significant feature of the*

case lies in its clear evidence that the energy of impact was chiefly transformed into lateral thrust and resilience of quasi-explosive type. Confessedly the most outstanding problem left is to find a source of energy adequate to the mechanical effects so impressively forced on attention. The case still remains something of a puzzle on that account. Meteoric matter has been found so widely disseminated through the débris, both within and without the crater, that the origin of the crater is no longer in doubt, but yet the amount of meteoric matter thus far brought into evidence seems clearly too small to be adequate. The suggestion of Barringer that the infalling mass was a cluster of meteorites or a comet's head is plausible in itself—and the orbits of comets are such as to make a bump into the earth a recognized contingency—but these suggestions give little help in the matter of adequacy. A larger mass than has been found seems to be required to satisfy the effects realized. For such computations as I have made, a siderite sphere 400 to 500 feet in diameter was taken, but it is scarcely worth while to give the results here. They are of the same import as those of the next case.

We ought not to overlook the fact that this is the only known case of such an infall in the history of the earth. This is an embarrassment in postulating a rapid series of infalls. Nor is its negative bearing merely a surface matter. If such a crater had been formed and buried in a natural way in any geologic formation, however old, there would be a fair chance of its detection. There is therefore a complete absence of geological warrant for supposing that infalls of this kind were ever anything but very sporadic affairs. If Meteor Crater was formed by the impact of the nucleus of a comet, theory would make its repetition an extremely rare event. The concept of an enormous meteorite, or close cluster of meteorites, other than cometic, has no observational basis. If the views respecting the origin of meteorites, later expressed in this article, have any cogency, the infall of such bodies would be governed by the same order of chances as those of comets. From no point of view, therefore, does Meteor Crater offer substantial ground for supposing that the earth was once molten because of the impacts of meteoritic bodies.

Case II. The questionable intimations of the craters of the moon.— The impact theory of the craters of the moon affords a concrete basis for the study of infalls of a still larger order. To fit this case, bodies of the order of five miles in diameter, more or less, seem to be required, and for working convenience these may be given the specific gravity of the moon, 3.34. The assumed size in this case has about the same ratio to the larger order of the moon's craters that the assumed 400 or 500 foot meteoritic body had to the size of Meteor Crater, but the mass is made relatively less to be in better accord with the moon's mass. The size is about the lower limit assigned to planetoids. No atmosphere can be supposed to have broken the effects of infall in this case or to have checked the free dispersal of the débris.

In the previous case there was surprisingly little evidence of liquefaction. What is the evidence here? The steep walls of the deep craters are quite incompatible with a liquid state, so far as this outermost part is concerned and this is the part subject to direct impact. There was strength enough in the crust to support the lunar Alps and Apennines, some of whose peaks tower to heights of 20,000 feet and more above the adjacent surface, i.e., 5,000 feet higher than their terrestrial prototypes. No less than ten mountain ranges have been recognized on the moon, which implies general crustal strength. The great relief of such elevations towering above such depressions is uncontrovertible evidence of strength and stability. The significance of this is emphasized, if the supposed impacts are made a part of the formative process of the moon, for then they are very old and have stood in this strong relief in spite of all the creep of the geologic ages.

A search for direct evidences of molten matter gives meager results under the most favorable interpretation that is tenable. Such of the craters as have level bottoms have been thought to imply a partial filling of lava, supposed to have risen from below after the craters had been formed. These bottoms may, however, be interpreted as level beds of clastic débris, like those that form the level bottom of Meteor Crater. So, also, the seemingly smooth, but really quite accidented, plains of the "maria" have been interpreted as great lava flows, but these may likewise be merely débris

plains. In the best photographs they are seen to have considerable relief and to be crisscrossed in different directions by lines of débris obviously shot from neighboring craters. They are thus at least surficially covered with clastic débris. But granting that everything which appears at this distance like lava really is lava, the whole does not imply a liquefaction of the moon of any other order than that signified by the great lava flows on the earth whose essential solidity is now beyond question.

But let us look at the question of rapid infall quantitatively and numerically. Let us assume that at the beginning of the accretion process, one-third of the mass of the moon was already in its core, while the remaining two-thirds had been gathered into bolides five miles in diameter which were yet to fall in. The mass of the moon is about 732×10^{17} tons. There would then have been 244×10^{17} tons in the moon-core and 488×10^{17} tons in the bolides yet to fall in. The mass of each of these bolides would have been about 997×10^{9} tons, and their total number about 49×10^{6}. Their individual volumes would have been a little over sixty-five cubic miles, while the volume of the moon-core would have been about 14×10^{8} cubic miles, and the radius of the core 708 miles. As the radius of the full-grown moon is 1,080 miles, the core would have had to grow radially 372 miles.

Now the surface area of the moon-core would have been 6×10^{6} square miles, while the disk of the five-mile bolides was a trifle less than twenty square miles in area, so that there would have been over 300,000 disk-areas on the surface of the moon-core. It would thus have required less than two hundred bolides to each disk-area to complete the full growth of the moon.

The liquid-forming impact theory now takes a critical form. We have seen that the surface of the moon shows that the last craters were not attended by general liquefaction or even a viscous state of their immediate walls. The last falls, however, were accelerated by nearly the full mass of the present moon, while the first falls were accelerated by only one-third the mass of the moon. The individual effects of the last infalls should, therefore, have been greater than any that preceded. They should also have inherited whatever benefits were transmissible from previous

infalls, in proportion to the time between falls. As these last impacts left no conclusive evidence of molten residue, it follows that no previous infall, in itself, can be consistently supposed to have left any greater molten residue and if their inheritance was greater it could apparently only come from a closer succession of infalls. Apparently, then, the only way in which a general molten condition can reasonably be supposed to have arisen was from the cumulative effects of such inherited residues of heat from the earlier infalls in excess of those of the later infalls. How tenable is this? There were by computation less than two hundred infalls of the specified kind to each disk-area during the whole accretion period of the moon. If that accretion period were essentially the same as that of the earth, as it should theoretically be, and if we compute the rate of infall by using the minimum accretion period assigned the earth based on mechanical and biological evidences, to make the rate as high as consistent, the mean interval between impacts would be about 3,000,000 years. If the mean accretion period had been used, the mean interval between infalls would have been more than twice this time. Very little inheritance of heat from a surficial bump can be postulated over an interval of this order.

But we are not left wholly to computations on estimated requirements. There is the direct evidence of the craters themselves. Some are fresh and their débris lines lie straight across older pits and older features of all sorts. Some pits and rims are worn or buried to the very limit of recognition, and there are all grades between. These features offer no warrant for the hypothesis that there was a closely crowded infall. They distinctly imply that the formation of the visible craters stretched over a long period. This evidence is the more cogent when the limited means of denudation, owing to the absence of an atmosphere and hydrosphere on the moon, are considered.

Now let us turn to the theory itself. If it be supposed that the five-mile bolides are planetesimals, the supposition itself hides under its cloak a quasi-assertion of the rate of their infall, for, as we have seen, all planetesimals started as very minute bodies controlled by a system of dynamics that imposed upon them slow growth as a

necessity of the limited amount of planetesimal matter, the large amount of space through which it was distributed, and the mutual relations of the planetesimal orbits, as already brought out. There were besides obstacles to growth beyond quite small sizes. Even if these obstacles be supposed to have been ineffectual, time for growth from the minute sizes to five-mile bolides must have intervened before the latter could function as crater-formers. They could thus have come into function only at a late stage. But accretion could not have been suspended in the meantime. They could therefore have come into function only as a *partial* source of lunar accretion. Growth from the smaller planetesimals must have gone forward during all the intervening period. Accretion simply by such giant planetesimals is thus incompatible with the fundamental conditions postulated by the basal hypothesis on which it rests.

The hypothesis is not much more promising if planetoids are substituted for the supposed giant planetesimals, for, by the mechanics of the case, the planetoids were given courses less favorable to aggregation than were the planetesimals, and hence greater intervals between their infalls must in consistency be assumed. This is in harmony with the observed fact that at least eight hundred planetoids are still following their own individual paths in a relatively limited tract and yet no collision or even dangerous approach to one another has been noted during the whole period of astronomical observation. In addition to this, we have found reasons for doubting whether planetoids could organize as nuclei of the planetary type under the differential stresses of the solar attractions that prevail in the region of the earth and in regions still nearer the sun.

The hypothesis that the pits of the moon were formed by the impacts of great meteorites offers no presumption that one infall would be followed by another in the same spot within any short period. As a cause of general melting, this is even more unpromising than the preceding.

The discussion thus far has proceeded on the assumption that the pits of the moon are the scars left by the impacts of great bolides of one sort or another. Before turning to the next topic,

it may be well to forestall misapprehension by making clear our view that such an origin of the craters of the moon is in itself improbable, for bodies moving in orbits under the control of the sun should plunge into the moon, if they strike it at all, at various angles to the vertical. In many cases the stroke should be quite oblique to the surface and should leave elongated pits, unsymmetrical rims, unequal dispersions of débris, and other tell-tale features; all the more so because the moon had no atmosphere to retard and turn downward the path of the bolides. Apparently the only escape from these grave objections lies in supposing that the explosive reaction was so great that it completely overwhelmed the effects of the direct stroke. If this assumption were tenable, it would seem to imply that the explosive dispersion was so great that it must have scattered all mobile matter, and especially all liquid matter so effectually as to insure its cooling while in flight.

THE SIGNIFICANCE OF THE EXPLOSIVE PHENOMENA OF THE MOON

The moon seems to have been a paradise of Krakatoas and Katmais. Interpreted as the product of gaseous explosion, the abundance and the greatness of the craters of the moon carry special significance. They have commonly been thought to imply a once molten state of the moon. I think the argument lies in precisely the opposite direction. If the moon, in its formative stage had been a molten globe, its high temperature should have set free all gases susceptible of being freed by any temperature that ever arose afterward. Its liquid state and its convective circulation would have brought these gases to the surface and given them opportunities of escape never equaled later, for the high temperature of the surface would have forced unsurpassed molecular activity and have insured their escape from the control of the moon. Even the cold full-grown moon cannot hold the volcanic gases. After all such gases had been boiled out of the moon and had escaped, and the gas-free lava had cooled, the moon should have been devoid of the means of explosive action.

On the other hand, if the moon were built up of minute clastic particles which carried such amounts of occluded and combined gases as meteorites do, and as they naturally would from their

long flights in the ultra-atmospheric field of the sun, and if the porous surface of the moon received and held by adsorption, chemical combination, or otherwise, molecular planetesimals of the gas-forming order, such as would inevitably plunge into it from the interplanetary field, there would be entrapped in the body of the moon, as it grew, a supply of disseminated gas-producing material sufficient to actuate great explosions whenever concentrated later by conditions favorable for such action. As the moon grew, its self-compression and the strains developed within it by neighboring bodies should have forced this potentially gaseous material toward the surface and developed the conditions of eruption. The moon should also have inherited its quota of radioactive substances and these should have played their part in the lunar vulcanicity. The fragmental constitution of the outer part of the moon, postulated as an inevitable feature of an accretional origin, should have rendered it specially susceptible to explosive effects. More or less local lava-production, as well as the quiet type of vulcanism, are entirely consistent with this view of the exceptionally gaseous eruptions, and they are postulated, but the evidence of the moon's surface seems to give this more quiet action a place quite subordinate to the gaseo-explosive phase.

THE SIGNIFICANCE OF TERRESTRIAL VULCANISM

The inferences that seem so imperative in the case of the moon apply also to the gaseous phases of vulcanism on the earth. The argument is a little less imperative because the earth is able to hold an atmosphere and would probably do so to some less extent in a molten state, and so, if it were once in that state, volcanic gases could have been retained in its liquid mass sufficient to balance the partial pressures of the like gases in such lessened atmosphere as the earth then held. The amount of gases so held in equilibrium could not have been large, and such as existed would not have served as explosive agencies because of the very fact that they were held in balance by opposing pressure. They were there merely because there was an outside pressure holding them there and that outside pressure was never removed under normal conditions. It seems merely declaiming the obvious to say that

such a gas content is incompetent to produce explosive eruptions and that such eruptions occur on the earth only when there are special developments or accumulations of gas within or beneath the exploded matter. In a molten earth, stirred during its long cooling stages by effective convection,[1] the gases set free by the various stages of heat of that period should have been so far brought to the surface and dissipated by molecular activity—except the limited equilibrium amount—that the earth when cooled and solidified should have been as deficient in explosive material as lavas are now found by experiment to be when melted in the open air at the surface and, after a long stage of boiling, cooled to the solid state. In essence, therefore, the case of the earth is the same as that of the moon. The studies of terrestrial volcanoes of recent years have brought forth accumulating evidence that volcanoes are actuated by inborn rather than outside gases, and that they are essentially independent of one another, though of course not independent of common conditions. Their explosiveness seems thus clearly due to their own individual resources and has no obvious dependence on any molten zone, sheet, pool, or other remnant of a once pervasive liquid state.

THE TESTIMONY OF THE ABERRANT BODIES OF THE SOLAR SYSTEM

We have now considered at some length the bearings of various lines of evidence drawn from the normal elements of the solar system. Let us turn for a moment to such suggestions as may be derived from the aberrant members of the system, the meteors, meteorites, and comets. If these are merely aliens that have been introduced incidentally from foreign sources, as some of them may be, there is little reason to expect them to teach much relative to the domestic organization; but if they were born in the system and are products of its dynamics, they may be quite as instructive as the normal members.

To discuss them with any definiteness, however, it is necessary to postulate the modes by which they came into being. These should reveal why they are aberrant, though products of the same

[1] See pp. 481–87 of previous article, this *Journal*, Vol. XXVIII (1920).

dynamics as the normal elements. I venture, therefore, to offer three hypothetical, but mutually consistent, ways in which meteors, meteorites, and comets may have arisen naturally and inevitably out of the dynamical system that gave rise to the planets as its normal product.

The problem of the meteors, meteorites, and comets is regarded as essentially one. Though complete demonstration has perhaps not yet been reached, it is assumed that meteors, meteorites, and comets are not only close of kin dynamically, but in some sense mutual derivatives. The spectacular phenomena which seem to put comets in a class by themselves are here supposed to be mainly the effects of the strongly contrasted conditions to which they are subjected at the extremes of their very elongated orbits. It is a suggestive fact that those comets which are supposed to have been reduced from extremely elongated orbits to shorter ones by the action of the great planets, show a notable tendency to lose their spectacular features and finally to pass by disintegration into meteor swarms. In the case of typical comets of extremely elongated orbits, a small loosely organized head—apparently a cluster of still smaller bodies held together by rather feeble gravitative control—swings from a relatively hot perihelion close to the sun to a very cold aphelion far out in space. During its long outer journey, all the constituents must become intensely cold to great depths and be liable to be deeply riven by shrinkage cracks, which, besides leading to coarse fragmentation, should facilitate the adsorption of molecules belonging to the sun's ultra-atmosphere. The action is supposed to be the same as that which gives to meteorites their occluded or combined gases. The outward swing of the comet occupies many years and often centuries, and there is time for even a very attenuated source of supply to furnish the requisite amount of gas-producing material.

When later the comet head, thus charged, approaches the sun, the gases are supposed to be set free by the solar heat on the sunward side, and to be driven forth toward the sun. At the same time, this differential heating is supposed to give rise to rapid and rather violent exfoliation, hurling the dissevered chips to

considerable distances against the feeble gravity of the head. By collisions in the course of their flights, these develop a quasi-gaseous meteoritic swarm whose triturative action should give rise to products of dustlike fineness. Both of these processes would doubtless be attended by much electrical dissociation in which the negative electrons would escape and the positive remain attached, so that electric repulsion would tend to drive both gases and dust sunward until repellant action from the sun reversed the movement and drove the whole backward in the form of the comet's tail. This sketch is very inadequate, but it may serve to suggest ways in which the distinctive features of comets may arise. The nuclei of the comets' heads may be merely clouds or clustered groups of meteorite-like masses loosely assembled by their own feeble attractions, and so subject to easy deployment and reassemblage as conditions require.

If the spectacular features of comets may thus be reduced to the incidental effects of extremely elongated orbits, the way is cleared for explaining how the material of the meteors, meteorites, and comet-heads may have originated, how their highly elliptical orbits were given them, and why these orbits lie in all azimuths and the bodies in them revolve indifferently in forward and retrograde directions in contrast to the systematic, orderly, and concurrent habits of the planetary bodies.

1. The first hypothesis assumes that, previous to the genesis of the present planetary system, the sun had a system of secondaries of the type which it could generate *without the co-operation of any outside body*. The assigned principles of such generation are those rigorous deductions from the kinetic theory of gases on which orbital ultra-atmospheres are postulated.[1] This class of secondaries would be, in the nature of the case, of a very much smaller order than our present planets. The orbits of such bodies would be likely to be thrown into erratic courses by the near approach of the massive body to which the origin of our present planetary system is assigned. Such of these small bodies as were thrown into very long elliptical orbits were made to suffer great extremes of heat and cold and might thus, it is postulated, have taken on

[1] "Celestial Kinships," *The Origin of the Earth* (1916), pp. 101–2.

cometic features, for a time, and later suffered dispersion into meteors and meteorites.

If any of these ancestral secondaries had attained a notable size and happened to be disturbed so as to be drawn through the Roche limit of the sun, it might be disrupted and become a clustered group suited to serve as the nucleus of a comet. This, however, would not be likely to occur in many cases, and so appeal is made chiefly to the riving action of cold in the aphelion journey as the dependable cause for the disrupted character of the comets' heads, and the fragmental features which meteorites commonly show.

2. The second hypothesis assumes that forces of the kinds disclosed by the observations of Pettit on the solar prominences of May 29 and July 15, 1919,[1] projected solar gases and precipitates into the outer regions of the sun's sphere of control, where its attraction was feeble, and where the attractions of neighboring stars and star groups were relatively strong. During these outer flights, the pull of some star, group of stars, or other outside source of attraction drew the ejected masses aside from their normal paths sufficiently to cause them to swing by the sun on their return and thus be forced to take highly elliptical orbits. The planes of these orbits and the direction of revolution thus generated would be determined by the various deviating attractions, so that a system formed by a large number of such deviations would be very heterogeneous orbitally. High ellipticity would be a common characteristic. The principles that control aggregation, as previously sketched in this series of papers, would apply to the projected matter in all such cases. In so far as this matter retained self-control, it would assemble by the precipitate-aggregate method into clouds of aggregates, and these would usually be still more closely assembled into loosely organized bodies well suited to function as the nuclei of comet-heads. These would be subject to all the vicissitudes of temperature and of alternate absorption and evolution of gaseous material, sketched above, and so display for a time the spectacular features of comets, and ultimately be disintegrated into meteorites. In so far as the projected solar matter was too highly dispersed for mutual control, it should have passed

[1] *Astrophys. Jour.* (Oct., 1919), pp. 206–19.

directly into meteoritic matter of the minutest type. At present, meteoritic particles, assumed to be of this type, abound in interplanetary space in such prodigious numbers that many millions are picked up daily by the earth. The formation of precipitate aggregates, in the methods previously sketched, seems to furnish an apt explanation of the origin of chondrules and of the other minute integers that so largely make up meteorites. The collisions of these little bodies as they were entering into the formation of larger bodies, seem well fitted to account for the intimate brecciation, the minute specks of glass, suddenly cooled liquid drops, as well as the strange mixtures of stony and metallic matter, and other distinctive features of meteorites.

3. The third hypothesis is dependent on the pre-existence of the present planetary system. It supposes that the ejected solar matter passed so near some one of the more massive planets that it was thrown into an elliptical orbit in a way similar to the preceding and with similar results. A certain portion of the particles so diverted would take orbits of the planetary type, so far as their planes are concerned, but only a part of these would have a planetoidal degree of circularity. Other portions would have orbits whose eccentricities, orbital planes, and directions of revolution were as various as are those of meteorites and comets. Certain comets are known to have orbits definitely related to the giant planets. This relation is commonly interpreted as the result of reduction from larger and more eccentric orbits by the planet's influence. Without questioning the validity of this interpretation, it is not inconsistent to hold that in a part of such cases, the comets arose *de novo* from planetary action in the way here suggested. Most comets developed in this way would probably belong to the feebly developed evanescent type.

These three hypotheses are entirely consistent with one another and may all be true. They have the merit of being made to rest on the same dynamic basis as the planetary system itself. These hypotheses for the aberrant factors, when added to the planetesimal hypothesis for the normal factors, give a theoretical unity to the whole solar system.

Now, if these are the true lines of interpretation, *the masses of meteors and meteorites, and their methods of infall, throw a flood of light on the sizes and the modes of infall of the planetesimals, for, by this interpretation, they are bodies of like origin and like general conditions.* On a conservative estimate, there are 100,000,000 or more minute meteorites, so small as to be wholly dissipated in the upper air, for every one that is massive enough to remain a visible body until it reaches the earth. Of the latter, none are known to exceed a dozen feet in mean diameter. No meteorite has ever been seen to produce melted soil or rock by its impact. When collisions with bodies that have no atmosphere take place, local melting probably results. The glassy bodies common in meteorites may very likely be such products. But the retention of such heterogeneous structures as are common in meteorites implies that there has been no *general* liquefaction. In so far, therefore, as the testimony of the aberrant factors bears on the size, rate of infall, and liquefying power of their dynamic relatives, the planetesimals, it supports the view that these are small, and in other respects it is in close accord with the deductions hereinbefore drawn from dynamical considerations. It is in intimate harmony with the testimony of the normal factors of the system.

Professor F. R. Moulton and Dr. W. D. MacMillan have been kind enough to read the manuscript of this and the three previous articles (X, XI, and XII), and to make valuable suggestions and criticisms. They are not responsible, however, for the computations. These have been verified by Miss Daisy W. Heath.